

A Simple Lecture Experiment with Radium Rays.

WHILST preparing some experiments for a lecture on this matter, I found a very simple device to demonstrate the important fact that radium rays are very easily transmitted through a high vacuum; and I am not aware that it has been published before in this way. I had at my disposal the strongly acting compound of radium bromide which is prepared at Brunswick, in Germany; 10 mgr. were enclosed in a small box of ebonite with a mica cover having a diameter of 20 mm. This was put down in a Dewar's tube with vacuum jacket, as is commonly used in experiments with liquid air, and held in place by a stopper of cotton wool. The tube was then turned upside down in a little dish with some mercury, so as to obtain a perfectly enclosed space, and the radium rays could only get out by the vacuum walls or through a thick layer of mercury; by taking enough of this dense liquid the escape may be stopped altogether. Putting now a charged sensitive gold leaf electroscope at a distance of 5 cm. from the tube, a leakage instantly sets in, so as to cause the instrument to be wholly discharged in fifteen seconds. I also tried a vacuum jacketed tube with silvered walls, but though this affords much better protection against the heat rays, I did not detect any considerable difference with regard to the former experiment; the discharge was almost as quick, demonstrating that radium rays are not reflected to an appreciable amount. Even when the radium bromide was put into a large Dewar's silvered balloon of 5 litres capacity, wrapped in cotton wool, and enclosed in a wooden case, in which liquid air would be preserved during more than a fortnight, the charged electroscope came to zero in half a minute when it was placed very near to it. The experiments are effective and easily arranged.

L. BLEEKRODE.

The Hague, November 20.

Nuclei and Ions

It is perhaps ungracious to reply to a review. I appreciate very fully that in cases of papers like mine, which take an isolated position and are written by a man who is not infallible, the task of the reviewer is burdensome enough. But Mr. C. T. R. Wilson's summary of several years of my work (October 8, p. 548) seems to me unnecessarily captious, and I am obliged to answer in self-defence.

I will not quarrel with Mr. Wilson about the titles of my papers, or about references to my first paper ("Experiments with Ionised Air"). I have had occasion to come back to it myself since (*Amer. Jour. Sci.*, xv., 105; *ibid.*, 217), and shall presumably do so again.

Turning to the second paper ("Structure of the Nucleus"), the impression given is that my first chapter is superfluous. The particular direction in which Mr. Wilson thinks it superfluous, *i.e.* the determination of reciprocal relations in the number of ions and nuclei arising in any process, I consider of special importance, as I shall explain below. Apart from this, the gist of the chapter is the (to me) very interesting result that phosphorus as a nucleator suddenly bursts forth into maximum activity at about 13°. The smoke at higher temperatures is a degradation. If I had made these experiments earlier I should not have drawn the comparison between the number of nuclei and the number of ions which Mr. Wilson impales. Recently (*Amer. Jour. Sci.*, xv., 217) I have departed widely from this early result.

With regard to my work on coronas, I had hoped that any rational attempt at systematisation would at least be tolerated. It was something, I thought, to plough through so bewildering a display and to get the general lay of the land in that deceptive colour territory, to distinguish sharply between the axial and the coronal colours, to ascertain that even in the former case the particles are large in comparison with the wave-length of light. So far as I know a discrimination of the evidence obtainable from the steam jet and the condensation chamber has thus for the first time been given. Mr. Wilson, however, has no encouragement. He gravely doubts "whether the method can be made a trustworthy one." Unfortunately I did not know this, for I have since ventured to repeat the whole work (*Amer. Jour. Sci.*, xvi., 325, and a forthcoming paper in Boltzmann's "Jubelband"), with corrections of method

and calculations, obtaining suggestive periodic variations of the coronal apertures for a given colour and the sizes of the cloud particles. I have recently succeeded in catching, holding, and approximately measuring under the microscope the particles of the finest fog (beyond the largest green-blue-purple corona). Again, in a year's continuous observation by my coronal method of the atmospheric nucleation of Providence (lest this lead to "misconception," let me say that no theological bearing is implied), I have found the data useful (*Physical Review*, xvi., 193; xvii., 233).

My interpretation of the experiments on the diffusion of the nucleus is in error, but I have long since corrected it (fully in *Amer. Jour. Sci.*, June, p. 472, briefly in *Drude's Annalen*, August, p. 1144). Hence I do not find Mr. Wilson's belated comments particularly helpful. I was so fully convinced that the excessively slow diffusions observed could only be due to the motion of nuclei that I failed to see that the small coefficients of the hydrocarbon vapours would be virtually accentuated in large degree by the occurrence of diffusion from saturated to somewhat less saturated vapour. But this bad break is not of primary significance in its bearing on my work; the original purpose of these experiments with hydrocarbon vapours, which Mr. Wilson overlooks, was this:—If the ionisation accompanying nucleation is favourable to condensation, it should be particularly so, presumably, in the case of the vapour of an ionising solvent like water. Hence if non-ionising solvents like the hydrocarbons be substituted for water, the absence of effects attributable to ionisation might be discernible. No essential difference was detected.

In the following remarks relative to nuclei produced by shaking liquids, it is astonishing to find a faint note of approval, but Mr. Wilson does not intend that it shall be taken too seriously. "There is nothing new," he hastens to add, "that nuclei of this kind exist." Verbally, this may be true, but the implication of the whole paragraph is much broader. He does not point out, however, where I may find a prior succinct statement, identical with the view which I give for the persistence of the solutional nucleus.

My "extraordinary hypothesis," as Mr. Wilson calls it, is a critical alternative, put forward to ascertain whether it has been proved that ionisation has an immediate effect on condensation, or whether such condensation is not even now to be regarded as a mere question of the size of the nuclei. The hypothesis should, in the first place, be fairly stated. In any region of intense ionisation there must be a correspondingly marked tendency to synthesis. The nucleus is the stable result of this synthesis. What its structure is to be depends, therefore, primarily on the chemical ingredients of the medium out of which the nucleus is made. Given a definite medium, simple or complex, and one may anticipate a nucleus of definite size and a corresponding supersaturation needed for condensation. My contention is, then, that if nuclei are formed by the X-rays at the anode and the cathode, they are liable to be different, because the ingredients out of which the nuclei are to be compounded are different. If they do not vary in size but merely in number with the intensity of the radiation, this need be no more surprising than that the products of combustion remain the same within a wide range of temperature.

My reasons for this view may best be developed in connection with the case of phosphorus. Mr. Wilson dismisses it by stating, "The answer is simply that the nuclei causing the phosphorus clouds are not free ions like those produced by the X-rays." Let me explain why I fail to grasp the term "free ion." The phosphorus nucleus, as experiment shows, is always a relatively persistent body, while the initial ionisation is to an equal degree characteristically fleeting. Usually before the emanation has been made available for condensation, only a few per cent. of the initial ionisation is left. Meanwhile, the nucleation or condensational activity has suffered no commensurate decline (*Physical Review*, xvi., 288). It is probable that the whole series of condensations subsequently to be evoked follow in the absence of ionisation.

The case of water nuclei is in this respect almost the same, except that the initial ionisation (I shall venture to call it so, since it discharges both positive and negative

electrification; *cf. Amer. Jour. Sci.*, xv., 105) is rarely neutral as a whole. But it vanishes almost completely while the number of nuclei is relatively constant. In general, diminutions which are questions of seconds or minutes with the ions are more than questions of hours with the nuclei.

Just as in these cases there is no marked decrease of the number of nuclei while the ions all but go, so I have been unable to find any contemporaneous increase of number; and yet in my experiments with phosphorus and with water nuclei the activities of any generator for the simultaneous production of nuclei and of ions seem to increase and decrease together. I shall be able to state this more definitely at the conclusion of my present experiments on the efficiency of different types of water jets.

Finally, in my "Experiments with Ionised Air" (p. 12), I showed that in case of tests made with the steam jet, nuclei produced by the X-rays in atmospheric air were persistent in like degree with phosphorus and other nuclei. In fact, there was little difference in this respect among the nuclei examined. Nuclei produced in dust-free air, saturated either with water vapour or with hydrocarbon vapour, by the X-rays acting from without, retain the same order of persistence, whereas the ionisation is known to be fleeting. True, rubber stoppers and tubes made up a part of my condensation chamber, but in the case of water nuclei, at least, I can see no objection to this. The entire absence of electric field is always understood.

In all cases, therefore, the electrification vanishes and leaves a nucleus behind, sometimes larger, sometimes smaller. If, in any one of them, the nucleation and the ionisation vanished at the same rate, the case would be good presumptive evidence of their identity. But, to my knowledge, never does this occur. What justification is there, then, to call the phosphorus nucleus an "oxide," or if an oxide associated with ionised air, why does one not find the smaller air nuclei? I should answer that the phosphorus nucleus is the stable product of the initially ionised field. Again, why is phosphorus and dry air a more complicated system than air and water vapour under the action of the X-rays? Out of both systems eventually issues a stable nucleation. And why may one attribute to ionised air different condensational properties, according as positive or as negative ions are in question, without having first established that the corresponding air nuclei do not differ in size sufficiently to account for the condensational difference observed? Why may one condense on a nucleus from which the soul has fled and still be permitted to call it an ion? Why, indeed, does the nucleus persist after the ionisation has vanished; why does one not get back to dust-free air? My answer would be as in the case of phosphorus. As to water nuclei, I am much in doubt ever since I have been able to arrest the finest fog particles for examination whether the nucleus from shattered water is mere water dust. It seems to me, therefore, that electrification, if present simultaneously with nucleation, is an incidental accompaniment with no immediate bearing on the condensation produced, and for this reason I have in the above endeavoured to account for the nucleus at the outset chemically.

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I DO not think that any worker with ions or with condensation nuclei who may have read the papers on "Experiments with Ionised Air" and on "The Structure of the Nucleus" will consider my criticism unjust.

The latter part of the letter requires some reply. According to Prof. Barus, in all cases studied by him the nuclei were distinct from the ions, persisting long after the ionisation had disappeared. All that this proves is that he has not yet succeeded in observing condensation upon the ions, but only upon nuclei of another kind. According to my experiments (*Phil. Trans.*, vol. xciii. pp. 289-308, 1899), a fourfold supersaturation is required to cause condensation on the negative ions, a sixfold being required for the positive ions. To get such high supersaturations as these an exceedingly rapid expansion is required, and it is probable that the apparatus used by Prof. Barus is unsuitable for the purpose. In the presence of any considerable number of nuclei requiring inappreciable supersaturation

(as persistent nuclei always do) to cause water to condense upon them, it must be particularly difficult to reach the supersaturation necessary for condensation upon the ions. Such persistent nuclei always were present in Prof. Barus's experiments; his failure to get condensation upon the ions was thus to be expected. His results have no bearing, therefore, upon the interpretation of my experiments on the action of the ions produced by X-rays and similar agents on condensation (for in these experiments nuclei more persistent than the ions were absent), nor of the experiments upon the charge carried by the ions made by Prof. J. J. Thomson (*Phil. Mag.*, vol. xlvi. p. 528, 1898, and vol. v. p. 346, 1903) and by Dr. H. A. Wilson (*Phil. Mag.*, vol. v. p. 429, 1903) with the same form of rapid-expansion apparatus as was used by me.

I have never been able to produce by the action of X-rays nuclei other than the ions, but possibly very intense radiation may do so, as ultra-violet light certainly does.

C. T. R. WILSON.

Cavendish Laboratory, November 23.

Weather Changes and the Appearance of Scum on Ponds.

SOME experiments which I have been making during the last year seem to bear very directly upon the interesting phenomenon described by "Platanus orientalis" in your issue of November 5. These experiments show that numerous solid substances suspended or dissolved in water have, by virtue of their surface-tension relations, a marked tendency to accumulate at any surface separating water from gas (*vide Proc. Roy. Soc.*, August). Hence, by merely passing a stream of air-bubbles through solutions or suspensions of certain solids in water, it is possible to effect a considerable concentration of the dissolved or suspended solid in the upper layers of the liquid. Each bubble carries with it to the surface a load of solid particles, and leaves many of them floating there either as an ultra-microscopic "pellicle" or as a visible "scum." If a bubble is very minute, its load may be so great in relation to its volume that it may be entirely unable to rise, or may even sink. If, in these circumstances, the barometric pressure be diminished, the volume of the bubble increases in greater proportion than the surface-area, and therefore than the maximum load, with the result that numerous bubbles previously unable to ascend at once begin to rise towards the surface. If, during their ascent, the barometric pressure be sufficiently increased, at once they sink. If a vessel of water containing a sediment of sulphur or calcium soap, &c., be exposed to a sufficiently diminished air-pressure, the whole of the sediment will be seen to rise to the surface, the minute air-bubbles with their coating of solid acting like so many "Cartesian Divers."

In every ordinary pond gas-bubbles of various kinds are constantly being formed by the action of micro-organisms; in nearly every pond various solid substances, both organic and inorganic, possessing the required surface-tension relations, are present both in the mud and in suspension. The gas liberated will be constantly bringing scum-forming material to the surface, whether it rises in large masses or in small bubbles. Either a fall in the barometric pressure, or a rise in temperature, or an increase in the activity of the gas-producing organisms should therefore result in increase of the scum. It must, however, frequently happen that the scum is swept to one side by the wind or sunk by various mechanical disturbances.

It would be extremely interesting to learn whether by "decided change in the weather" your correspondent means a change attended by a falling barometer.

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W. RAMSDEN.

The "Affenspalte" in Human Brains.

Will you kindly allow me the privilege of using your columns for the following note? In a recent number of the *Anatomischer Anzeiger* Prof. Elliott Smith published a most interesting forecast of an extensive work which he has in hand, dealing particularly with the occurrence in human brains of an occipital operculum; this occurrence had been considered previously as very exceptional, but Prof. Elliott Smith is able to show that this is far from being the case.